in falling into the sun, and are comparable with the moderate little velocity (29·5 kilometres per second) of the earth in her orbit round the sun.

To fix the ideas, think of two cool solid globes, each of the same mean density as the earth, and of half the sun's diameter, given at rest, or nearly at rest, at a distance asunder equal to twice the earth's distance from the sun. They will fall together and collide in half a year. The collision will last for a few hours, in the course of which they will be transformed into a violently agitated incandescent fluid mass, with about eighteen million (according to the Pouillet-Helmholtz reckoning, of twenty million) years' heat ready made in it, and swelled out by this heat to possibly one and a half times, or two, or three, or four times, the sun's present diameter. If instead of being at rest initially they had had a transverse relative velocity of 1·42 kilometres per second, they would just escape collision, and would revolve in equal ellipses in a period of one year round the centre of inertia, just grazing one another's surfaces every time they come round to the nearest points of their orbits.

If the initial transverse component of relative velocity be less than, but not much less than, 1·42 kilometres per second, there will be a violent grazing collision, and two bright suns, solid globes bathed in flaming fluid, will come into existence in the course of a few hours, and will commence revolving round their common centre of inertia in long elliptic orbits in a period of a little less than a year. The *quasi*-tidal interaction will diminish the eccentricities of their orbits; and if continued long enough will cause the two to revolve in circular orbits round their centre of inertia with a distance between their surfaces equal to 644 of the diameter of each.

If the initial transverse component relative velocity of the two bodies were just 68 metres per second, the moment of momentum, the same before and after collision, would be just equal to that of the solar system, of which seventeen-eighteenths is Jupiter's and one-eighteenth the sun's: the other bodies of the system being not worth considering in the account. Fragments of superficially-melted solid, or splashes of fluid, sent flying away from the main compound mass could not possibly by tidal action or other resistance get into the actual orbits of the planets, whose evolution requires some finer if more complex fore-ordination than merely the existence of two masses undisturbed by any other matter in space.

I shall only say in conclusion:—Assuming the sun's mass to be composed of portions which were far asunder before it was hot, the immediate antecedent to its incandescence must have been either two bodies with details differing only in proportion and densities from the cases we have been now considering as examples; or it must have been some number more than two—some finite number—at the most the number of atoms in the sun's present mass, which is a finite number as easily understood and imagined as number 3 or number 123. The immediate antecedent to incandescence may have been the whole constituents in the extreme condition of subdivision—that is to say, in the condition of separate atoms; or it may have been any smaller number of groups of atoms making up minute crystals or groups of crystals—snowflakes of matter, as it were; or it may have been lumps of matter like this macadamising stone; or like this stone, which you might mistake for a macadamising stone, and which was actually travelling through space till it fell on the earth at Possil, in the neighbourhood of Glasgow, on April 5, 1804; or like this—which was found in the Desert of Atacama in South America, and is believed to have fallen there from the sky—a fragment made up of iron and stone, which looks as if it has solidified from a mixture of gravel and melted iron in a place where there was very little of heaviness; or this splendidly crystallised piece of iron, a slab cut out of the celebrated aërolite of Lenarto, in Hungary;[1] or this wonderfully shaped specimen, a model of the Middlesburgh meteorite, kindly given me by Prof. A. S. Herschel, with corrugations showing how its melted matter has been scoured off from the front part of its surface in its final rush through the earth's atmosphere when it was seen to fall on March 14, 1881, at 3.35 p.m.

For the theory of the sun it is indifferent which of these varieties of configurations of matter may have been the immediate antecedent of his incandescence, but I can never think of these material antecedents without remembering a question put to me thirty years ago by the late Bishop Ewing, Bishop of Argyll and the Isles: "Do you imagine that piece of matter to have been as it is from the beginning; to have been created as it is, or to have been as it is through all time till it fell on the earth?" I had told him that I believed the sun to be built up of stones, but he would not be satisfied till he knew or could imagine, what kind of stones. I could not but agree with him in feeling it impossible to imagine that any one of these meteorites before you has been as it is through all time, or that the materials of the sun were like this for all time before they came together and became hot. Surely this stone has an eventful history, but I shall not tax your patience longer to-night by trying to trace it conjecturally. I shall only say that we cannot but agree with the common opinion which regards meteorites as fragments broken from larger masses, but we cannot be satisfied without trying to imagine what were the antecedents of those masses.

PROTOPLASM[2]

It is a natural and beneficial result of the present energetic pursuit of biological science that every now and again some thinker comes forward to show us where we stand, and to what our thoughts are impelling us. Subordinate to the universal eminence and influence of a Linnæus or a Darwin, the critics of a decade exert no small effect on contemporary investigation by suggesting new modes of viewing or expressing things; and even though the originality is not always happy, and the generalisations are sometimes unfortunate, it is nevertheless a healthy sign that specialists of reputation, led to view matters with a severely critical eye as their work progresses, occasionally turn round and warn us that it would be as well to take stock of the facts, and see what are the chances of solving some large problem. Moreover, it has to be borne in mind that as various branches reach a certain stage their results need overhauling by specialists in other departments, and it becomes a question who is to prepare the problems of biology, for instance, so that the mathematician or the physicist may criticise them.

As much on this account as for his own contributions to the store of facts, we must welcome Dr. Berthold's clever "Studies" as an earnest and important attempt to contribute to a knowledge of the mechanics of life. Of course it is always a difficulty to decide how far a specialist may be expected to take an accurate view of a large problem to the direct solution of which his own researches can contribute but little; but experience has shown that more is to be looked for from the deep insight obtained by close investigation than from the few brilliant suggestions scattered through volumes of merely clever thinking. In the present case, the moderate tone of the book, and the easy earnestness of the writer, should at least insure careful reading of the 324 pages of text in which Dr. Berthold expresses his bold ideas; and whether the conclusions stand or fall, the reader will be amply repaid by the observations collected and the criticisms on several questions now agitating the minds of botanists.

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[1] The three aërolites now exhibited belong to the Hunterian Museum of the University of Glasgow, and have been kindly lent me for this evening by the Curator, Dr. Young.

[2] "Studien über Protoplasmamechanik." By Dr. G. Berthold, Professor of Botany in the University of Göttingen. (Leipzig: Arthur Felix, 1886.)

The title of the book must be taken in a comprehensive sense, for the whole "cell-theory" (if we possess one) is under review in the nine chapters into which the text is divided. This extensive aim is justified by the author's treatment of the subject, which affords an admirable survey of current botanical speculation.

Before proceeding to a closer examination of the work, we may state that there are seven well-executed plates, with descriptions which are too short. The index might have been more ample—it includes the names of plants and authors only—but the table of contents is very full and good.

An introduction of eleven pages leads us at once to the chief position assumed by the author. So long as we do not understand the *Amœba*, we must be in the dark as regards the mechanism of life in higher organisms. Nothing has been gained by regarding protoplasm as "living proteid," or as containing "living" as opposed to "dead" proteids, and so forth: moreover, no clearness, but rather the contrary, has so far resulted from hypotheses as to the "structure" of protoplasm, or from distinctions between "idioplasm" and other constituents. The author therefore inquired whether we are not perhaps treading an aimless path, and whether we should not go back and examine earlier views, and proceed anew. The consequence to his mind was the resumption of the old analogy between a drop of protoplasm and a drop of fluid, and he was led to inquire into the analogy more deeply, especially on finding that a detailed analysis of the problem had not before been seriously undertaken. As the general result of investigations begun in 1882, the author decides that protoplasm is to be regarded as a highly complex emulsion, differing in consistence in the different cases. There is nothing in the chemistry and metabolism known which need clash with this view of the fluid nature of the "physical basis of life," and the author decides that the forces upon which the changes of form, internal movements, and so forth, depend, are the same as those which determine whether a fluid shall assume the form of a drop, or drops, or spread out and wet another body, and so on—in fact, the forces concerned in surface-tensions.

The author frankly admits the difficulty, and even seeming impossibility, of imitating some of the conditions, or even of deciding whether the actions of protoplasm accord with the theory. This must naturally be the case; and of course no one expects him to imitate all the conditions experimentally. The method employed is essentially deductive and analytic throughout, and for this reason the greatest possible care must be employed in taking any step forward. Partly on this account, and partly owing to other circumstances, the book needs cautious reading, and great difficulties will be felt in regard to many points. This is apart from an undoubted (though perhaps unavoidable) blemish in the book, which consists in the author so often putting off for some pages the consideration of a subject commenced.

The key-note, as it were, of the work having been indicated, a few words must be said regarding some leading features in the various chapters. The first subject dealt with is the layered or stratified nature of the typical cell. A spore of *Equisetum*, for instance, may be regarded as a system of concentric layers. First there is a central nucleus; then various layers of protoplasm, of which the innermost is colourless and contains certain minute granules, the second is thicker and carries the chlorophyll-corpuscles, the third is hyaline and contains lenticular refractive bodies of peculiar nature; then follows the cell-wall, if nothing further. The cell-wall is usually composed of three or more layers.

If we consider the cells of a tissue, Berthold points out that a given partition membrane must be regarded as dividing and belonging to two symmetrical plasmatic systems, and as being their middle and innermost layer.

But all cells are not systems of concentric layers. Not only are excentric layers found, but a complexity is introduced as soon as the sap-vacuoles appear, in many cases making the cell not monocentric but polycentric. The *normal* order of the layers, as exemplified by the spore of *Equisetum*, or any simple cell with one large vacuole, &c., may be distinguished from the *inverse* order exhibited, for instance, by the cords in a *Caulerpa*, or the central mass in a cell containing raphides, or anywhere where the sap bathes the system of layers referred to.

It is then shown that in many cases where oil-drops, &c., have usually been regarded as lying free in a cell, they are inclosed in an ingrowth from the cell-wall, reminding us of cystoliths. An examination of intercellular spaces follows: the most interesting question is as to the existence of protoplasm in lacunæ between cells. Berthold quotes *Aconitum Napellus* as affording conclusive evidence, and confutes the contention of Gardiner and Schenck against Russow's statements. Berthold goes much further, however. He finds a thin layer of protoplasm overlying the cuticle of the epidermis and of spores, and, to put it shortly, concludes that the cell-wall is formed and embedded in protoplasm, and not excreted on its surface—the cell-wall is a supporting apparatus, not a protective one. Again, a cell forming part of a tissue cannot be forthwith compared with a unicellular Alga, for this reason: the latter may be regarded as consisting of two parts, (1) the inner protoplasmic system with its contiguous share of cell-wall, (2) the outer strata of cell-wall *plus* the hypothetical covering of protoplasm. Only the first of these two parts of the algal cell can be compared with a tissue-cell.

The relation of these ideas to Sachs's view, that we are to regard a plant as a whole cut up into cell-chambers, and not as a whole built up of single cells, is obvious to all who have followed recent speculations in botany.

It is, of course, impossible to go at any length into the contents of all the chapters. The second is concerned with the finer structure of the cell—nucleus, chlorophyll-corpuscles, and other cell-contents. Incidentally we may note the emphatic statement that starch is *not* formed in the *Melanophyceæ* (p. 57); that the word "microsome" has no definite meaning, and had better be discarded (p. 61). Later on the author expressly states his inability to confirm Strasburger's and Schmitz's conclusions that microsomes are employed in building up the cell-wall (p. 208), and even hints at confusion between crystalline particles and microsomes in the case of *Spirogyra!*

If protoplasm is an emulsion, it follows that the various processes of separation of sap-vacuoles, oil-drops, crystalline and other particles, have to be explained as according with similar separations in lifeless mixtures. Berthold finds no difficulties insuperable here, and even discusses the probable origin and disappearance of chlorophyll-corpuscles and nuclei on the assumption that they are part of the protoplasm. Although they now always arise by the division of those previously existing, they must have been formed from protoplasm in the first instance. The action of external stimuli offers a fertile subject for discussion. As regards geotropism, the author regards "the primary effect of gravitation' as consisting in the different rates of movement of substances of different specific gravity.

The supposition that anything is explained by regarding protoplasm as essentially "living proteid," is severely criticised on pp. 74 and 75, and the author agrees with Baumann that the arguments which exalt proteids into the position of being the most essential constituent of protoplasm would apply equally well to water. The "living substance of organisms" is always an extremely complex mixture. At the same time, it would seem that the author here raises some gratuitous difficulties, since no biologist really regards protoplasm as a simple substance, proteid or otherwise. One consequence of the discussion might almost be foreseen: Berthold proposes to recast the definition of protoplasm, and to subordinate to it—the fluid mixture absent from no living cell—cytoplasm, nucleus, chlorophyll-bodies, vacuoles, tannin and oil-drops, &c., as so many parts, of the protoplasm as a whole. He urges that the introduction of the ideas cytoplasm, ectoplasm, and so on, have driven the time-honoured word protoplasm out of the field, whereas its usefulness as a comprehensive word—though with a somewhat different meaning from the current one—for the whole is undoubted. Moreover, it is to be insisted upon that the protoplasm is active as a whole.

The discussion as to the meaning of the term "organised" must be here passed over, with many other points of interest.

In the third chapter we have a long analysis of the movements of naked masses of protoplasm. All turns upon the tendency of a mass of protoplasm to assume the form of a spherical drop; this can only be due to the same causes which impel a drop of any accepted liquid to assume the drop condition. Justice could not be done by summarising this analysis, and the demonstration that cylinders of protoplasm, like cylinders of liquids, tend to break up in a definite way. The end result of a long argument is, that the amœboid condition depends upon the degree of wetting of the environment by the fluid protoplasm and *vice versâ*.

If three fluids which do not mix are in contact with one another, the tensions at their surfaces can be mathematically investigated, and Berthold maintains that the principles here concerned govern the behaviour of a drop of protoplasm as they do that of an ordinary liquid under the given conditions. The phenomena of spreading out, putting forth and withdrawing pseudopodia, rounding off, &c., are due to the same causes and ruled by the same laws as the flowing of one liquid over another, or its withdrawal from it (glycerine and alcohol *e.g.*), or its assumption of the drop form, and so on. Of course amœboid movements are complex, because the liquid amœba is not a simple fluid, but is undergoing rapid changes due to its metabolism and exchanges with the environment, processes which are acting with different energy at different places. It must be clearly understood that a rapid survey of Berthold's position cannot do justice to his argument: whether his position is accepted or rejected, there is no doubt that he clearly sees and provides for many important difficulties, some of which seem to have been overlooked. It will be regarded as a startling idea by some (though the idea is not altogether new) that fine pseudopodia are not the results of activity on the part of the amœba: such pseudopodia must be looked upon as *drawn out by* the surrounding medium, not *put out into* it. Here, again, exceptions occur where blunt processes are driven forth by local contractions and other causes, but the sum total of all the argument is (as expressly stated again on p. 109) that the amœboid condition is the symptom that the organism wets the substratum and displaces the surrounding medium, indicating that the intensity of the tension between the medium and the protoplasm is but small. The discussion as to the causes and effects of the internal movements in protoplasm must here be passed over, with the simple remark that the author sees no difficulty which cannot be explained from our knowledge of the mechanics of liquids. On p. 106 is proposed an explanation of the remarkable filaments observed by F. Darwin on *Dipsacus sylvestris*.

Chapter IV. deals with what is practically a continuation of the second chapter—the symmetry or arrangement of the cell-contents. The stratified or shell arrangement is again expressly referred to, and an attempt made to explain it on the main assumption of the book. The arrangement referred to is a consequence of exchanges (diffusion, absorption, &c.) with the environment: passive particles suspended in the cell would have to assume positions which are definite; active particles (*i.e.* particles which themselves exchange with the layer in which they are embedded) might interfere with the simple shell arrangement, and we have systems within a system. After examining what occurs in the case of a spherical system or cell, the author extends the analysis to an ellipsoid and other anisodiametric systems, and finds the results accord with what is found in Nature. The question of the "Hautschicht" is then attacked, and De Vries' late statements as to the existence of a pellicle or "wall" around the vacuole are criticised. Berthold, to put it shortly, condemns this pellicle as an artificial product—a "precipitation-membrane"—in many if not in most cases. On p. 154 it is still more emphatically stated that the cell-wall inside the cell is formed "always in the interior of the protoplasm, never on its surface," and it is probable "that the same is the case even when free masses of protoplasm surround themselves with a membrane." The membrane stated to exist around the nucleus is condemned, with a certain reserve, as a probable precipitation-membrane. Other interesting points must be passed over.

The fifth chapter is practically concerned with showing that in spite of the great variety of forms exhibited by the chlorophyll-bodies of different plants, especially Algæ, their position, consistence, changes in form, division, &c., can be explained in accordance with the view that they are parts of an emulsion. Other cell-contents are considered also—oil-drops, tannin, nucleus, vacuoles, &c.—but at less length. The chlorophyll-corpuscles of higher plants are compared to drops resting on a substratum which they do not wet, their shape being in part due to radial pressures. When they are more extended and amœboid, their actions are explained according to the principles (contact of three surfaces, &c.) employed before. *Spirogyra* and other Conjugatæ present difficulties. While the "chlorophyll-apparatus" displays a relatively large surface, the converse is the case with nuclei and other cell-contents, and the form of the spherical drop (maximum cubic contents with minimum superficial area) is usual; though exceptions exist and are investigated.

The division of chlorophyll-corpuscles is then examined, and this leads naturally to the division of the nucleus and cell, which is treated separately. A spherical mass of fluid must increase its surface if it divides: this implies a diminution of tension at the common surface (as with the formation of pseudopodia), and concentric shells in the medium or in the mass of fluid in question. All the conditions fulfilled, pseudopodia can be formed either from the medium into the mass, or from the mass into the medium. An annular pseudopodium would divide the spherical (or spheroidal) mass into two. This is, shortly put, the position as Berthold views it. He then again applies the analysis of dividing cylinders, and proceeds to apply the results to what is observed in a cell. The radial pressure, and growth in one direction of the cell, may be important factors. But the real difficulty is met with when considering the division of spherical bodies in the cells of the growing-points, for instance; and the same applies to cell-division. Why should a sphere—a stable form—pass over into an elongated body, which then divides? It must be assumed that "under the influence of its own metabolism, and that of its environment a bi-polar symmetry arises in the chlorophyll-corpuscle, in consequence of which the division takes place equatorially."

This leads to the sixth chapter, which is in many, respects the most important, as it is the most interesting. After reviewing the process of cell-division generally, the author separates the essential from the unessential processes, and agrees with Strasburger that the division of the nucleus must be regarded as an accompanying phenomenon. The division of the ovum of *Echinus* and *Ciona* is described: soon after the male and female nuclei have fused, two centres appear in the egg, each with radii—the required bi-polarity is established. The exchanges and movements in the protoplasm are then followed; the result is that certain constituents accumulate to excess in the equator between the two radiating centres, or "suns." The chief points are illustrated by diagrams. The two "suns" are the centres of the future daughter-cells; the still single nucleus lies between them in a bridge of the same protoplasm as the "suns" (these "suns," by the bye, are the *Attractions-Kugeln* of Van Beneden, and the *Pol-Kugeln* of others) are embedded in: the more peripheral protoplasm of the cell (ovum) has accumulated chiefly around the nucleus—*i.e.* in the equatorial plane. This equatorial protoplasm then begins to cut in two the nucleus, which has assumed the "karyokinetic" condition. Passing over many details, we may sum up the explanation shortly. The superficial shells of protoplasm are assumed to put forth pseudopodia between the "suns"—*i.e.* the author regards it as fundamentally a wetting process, due to changes at the surfaces. The processes are essentially of the same nature in vegetable cells, though it is impossible in a short space to summarise Berthold's discussion as to the relative importance of the numerous details which occur in different cases. Obviously the stumbling-block which is best worth further attack is the origin of bi-polarity in a spherical mass: that Berthold's suggestions do not satisfy the requirements will probably occur to everyone. The explanation offered to account for the complex karyokinesis cannot be regarded as fully satisfactory. At the same time some advantage may accrue from the new lights in which he puts the central figures of cell-division. We are here only half through the book however, and must proceed, confining our remarks still more closely.

Chapter VII. treats of the cell-network of plants, and the directions of cell-divisions, &c. It is in great measure a criticism of Sachs's celebrated view of the structure of the higher plants, and deals at some length with several of his positions. Of course, Berthold assumes primarily that the plant is to be regarded as chambered—cut up into cells, not built up of them. Two main principles are then employed. (1) The cell-divisions are, as a rule (at least in growing-points, &c.), halvings—*i.e.* each daughter-cell has the same cubic contents. This leads to a discussion of very many cases. Of course the shape of a segment does not forthwith enable us to judge of its relative contents, and difficulty occurs sometimes on this account: it is impossible to summarise the remarks, and especially since reference to the figures is necessary. (2) The second fundamental principle is that which regulates the position of fluid lamellæ elsewhere—the principle of least areas. The rule is that the new cell-wall takes such a direction that its area is the smallest possible. There are exceptions, *e.g.* cambium cells; but at least one feature appears to indicate a tendency to follow the principle—cell-walls never abut in the angles of cells. Sachs's law of rectangular division is comprehended as a particular case of Berthold's more general law: it fails where simultaneous divisions result in the formation of polygonal cells—*e.g.* in the embryo-sac—with walls inclined at angles greater than the right-angle.

The eighth chapter deals with the sculpturing on the interior of cell-walls, and allied phenomena; while Chapter IX. (the last) is devoted to "free cell-formation."

Enough has been said to show the wide scope of the book, though full criticism of it will only be possible after some of Berthold's test-cases have been worked over. Of course, from the nature of the work, it is open to the charge of being transcendental; but at the same time it must be allowed that we are getting into serious difficulties with protoplasm, and good, bold, shaking criticism is beneficial. In any case, several investigators will, no doubt, have something to say to Berthold's statements, for there is no lack of observations, old and new, as well as hypothesis, in the book we dismiss with this short review.

H. MARSHALL WARD

ON THE EXPLOSION OF METEORITES

WE have received from M. Hirn a *tirage à part* of a communication to *L'Astronomie*, in which he discusses the various phenomena accompanying the explosion of meteorites, with a view to explaining their causes.

M. Daubrée, a long time ago, pointed out how very striking and difficult of explanation the noises are which are often heard in connection with the passage of meteorites, and called in question the explanation which had been given of their being really due to a veritable explosion.

M. Hirn, in his paper, begins by considering the causes which are at work in the production of the thunder which accompanies electric discharges, and of this he writes as follows:—"The sound, which we call thunder, is due, as everybody knows, to the fact that the air traversed by an electric spark, that is, a flash of lightning, is suddenly raised to a very high temperature, and has its volume moreover considerably increased. The column of gas thus suddenly heated and expanded is sometimes several miles long; as the duration of the flash is not even a millionth of a second, it follows that the noise bursts forth at once from the whole column; but for an observer in any one place it commences where the lightning is at the least distance. In precise terms, the beginning of the thunder-clap gives us the minimum distance of the lightning; and the length of the thunder-clap gives us the length of the column. It must be remarked that when a flash of lightning strikes the ground, it is not necessarily from the place struck that the first noise is heard." M. Hirn then gives an interesting case which proves the truth of this remark. He next points out that a bullet whistles in traversing the air, so that we can to a certain extent follow its flight; the same thing happens with a falling meteorite just before striking the earth. The noise actually heard has been compared to the flight of wild geese or the sound produced when one tears linen: it is due to the fact that the air rapidly pushed on one side in front of the projectile, whether bullet or meteorite, quickly rushes back to fill the gap left in the rear.

The most rapid cannon-shots scarcely attain a velocity of 600 metres a second, while meteorites penetrate the air with a velocity of 40,000 or even 60,000 metres per second; and this increased velocity gives rise to phenomena, which, although insignificant where cannon-shots are in question, become very intense and important when we consider the case of the meteorite. With that velocity the air is at once raised to a temperature of from 4000° to 6000° C. The matter on the surface of the meteorite will be torn away by the violence of the gaseous friction produced, and will be vaporised at the same time by the heat. This is undoubtedly the origin of the smoke which meteorites leave trailing behind them.

We have, then, precisely as in the case of lightning, a long narrow column of air, which is expanded, not so instantaneously certainly as by lightning, but at all events in an extremely short time and through a great length. Under these circumstances we should have an explosion in one case as in the other: a clap of thunder followed by a rolling noise more or less prolonged. If a cannon-ball could have imparted to it a velocity of 100,000 metres per second, it would no longer whistle, it would thunder, and at the same time it would produce a flash, as of lightning, and would be instantly burnt up. M. Hirn depends upon this line of reasoning to show that meteoric thunder need not necessarily have anything to do with an actual explosion. He then points out that the intensity of the noise produced in every point of its trajectory